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I. *Two Letters from the Rev. Mr. Granville Wheler, F. R. S. to the President, concerning a rotatory Motion of Glafs Tubes about their Axes, when placed in a certain Manner before the Fire.*

I.

S I R, London, Mar. 28. 1745.

Read on March
28. and April 4.
1745.

ABOUT four Years ago, Mr. Charles Orme, of *Ashby de la Zouch* in *Leicestershire*, acquainted me, That, in drying his glafs Tubes for his diagonal Barometers (which for some Years he has continued to make in much greater Perfection than any other Person that I know of in *England* *), he had observed a rotatory Motion about their *Axes*, and at the same time a progressive one towards the Fire. He was so obliging then as to promise at any time to shew me the Experiment; but other Business intervening, I still deferred accepting his Offer; having the less Curiosity to see it, as I imagined the Motions were occasioned by the Draught of Air up the Chimney, assisted by the Weight of the inclining Tube. But a little above a Year ago, making some Stay at *Ashby*, upon repeating his Offer, I went to see the Experiment, which answered fully to his Description: The Tubes, which were about four Feet long, and half an Inch over, moving at six or eight Inches Distance from the Fire, not only progressively, and about their *Axes* along the Side-Wall they lean'd against, but along the Front-Wall

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of the Chimney, which made an obtuse Angle with the other ; so that they seemed to move up hill, and against their Weight.

Surprised at this, I thought the Case deserved a little farther Examination ; and proposed placing two Tubes horizontally, parallel to each other, and at right Angles to the Face of the Fire, to be Supporters to a third, which was to be placed upon them parallel to the Fire. We did so, and with Pleasure observed the supported Tube turn about its *Axis*, and move on towards the Fire in such a manner, as made me still less inclined to think either of the Motions owing to the Draught of the Fire, and certainly not to the whole Weight of the moving Tube ; a fine Spirit-Level informing us, that the supporting Tubes lean'd from the Fire ; so that the Motion was a little up-hill.

This Success determined me, with Mr. *Orme's* Leave, to go on farther ; and, furnishing myself, from him, with Tubes of several Lengths and Thicknesses, I made several Trials ; and found, that with a moderate Fire the Experiment succeeded best, when the supported Tube was about twenty or two-and-twenty Inches long, the Diameter about one Tenth of an Inch, and had in each End a pretty strong Pin, fixed in Cork, for an Axe to roll with upon the supporting Tubes ; which, to lessen the Contact, had nearly the same Diameter with the moving one. Under these Circumstances the Tube would begin to move at eighteen Inches Distance from the Fire ; and continue to do so, with little Intervals, till it touched the Bars ; and moved much in the same manner, when a little Ball of Cork, an Inch or more in Diameter,

meter, was fixed in the Middle of it. But what surprised me still more, and seemed to take off the Objection of the Draught of the Chimney, was, letting it once stay a little while against the Bars, I found it still continue its Motion about its *Axis* in the same Direction.

This put me upon making little Rings of Wire, to fix upon and move along the supporting Tubes, so as to stop the moving Tube at any Distance from the Fire I pleased.

Stopp'd with these, the Motion of the Tube about its *Axis* still continued.

Desirous to try what would be the Effect in or near an upright Posture, I made the Pin at one End of my Tube rest upon a *China* Plate, that at the other Turn in a silver Socket (that carried my Pencil) fixed in an horizontal Arm of Wood, but so as I could slip it up and down, to adapt it to the Length of of the Tube. Here I found, that if the Tube lean'd to my Right hand, (which was the Case of Mr. *Orme's* Tubes before his Fire) the Motion was from East to West; but if they leaned to my Left, the Motion was from West to East; and the nearer I could get to the perfectly upright Posture, the less the Motion seem'd to be either Way.

I now proceeded to place my Tube horizontally upon a glass Plane (a large Fragment of a Coach-side Window Glass). The Tube, instead of moving towards the Fire, moved from it, and about its *Axis*, in a contrary Direction to what it had done before, Observing that this glass Plane was broader at one End than the other, and that the Rotation backwards was more sensible when the narrower End was

towards the Fire, I placed a triangular Piece of the same Glass with its *Vertex* towards the Fire nearly horizontal, but rather rising from the Fire; so that its Base was a little higher than its *Vertex*; and upon it a Tube of Glass, about 22 Inches long, and $\frac{1}{8}$ of an Inch Diameter, near the *Vertex* and the Fire. This Tube receded from the Fire, moving about its *Axis* till it came to the Distance of eight Inches; which is four Inches more than it receded the Day before upon the same Piece of Coach-Glass, before it was broke into this triangular Form.

I was naturally led now to make use of two supporting Tubes, instead of the triangular glass Plane. These were about eighteen Inches long each, and $\frac{1}{8}$ of an Inch in Diameter, and placed parallel to one another at the Distance of about two Inches, so as to support the moving Tube near the Middle of it. When very nearly horizontal by the Level, the supported Tube moved from the Fire about its *Axis* to the Distance of thirteen Inches: When the Supporters were a little raised at their remote Ends, so as manifestly by the Level to descend towards the Fire, it receded to the Distance of ten Inches, moving as before about its *Axis*; but in this latter Case the Fire had declined a good deal; otherwise, probably, the Tube would have receded farther, tho' up-hill.

The next Day, the same Tube, when the same supporting Tubes were $8\frac{1}{2}$ Inches distant from each other, receded nearly as before: When $12\frac{1}{2}$ Inches from each other, it stood still; and when removed to the Distance of $16\frac{1}{2}$ Inches, the supported Tube very manifestly changed its Motion, and went towards
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the Fire; as it did afterwards, when the Inclination of the supporting Tubes was alter'd, so as to ascend towards the Fire.

I made several other Experiments, with regard to the Situation of the Tubes to the Fire, with regard to the Quantity of Fire suffered to come at the Tubes, and with regard to Attraction and Repulsion, which I will not trouble you with at present: Only observe, that, when the Tube had four others under it, all supporting, one near each Extremity, and one on each Side of its Centre, no Motion at all was perceived; and when two of them on the same Side of the Centre were taken away, the supported Tube moved into an oblique Situation with regard to the Fire, the unsupported Half receding from the Fire.

Upon the Whole, it appears sufficiently plain, that the Stream of Air up the Chimney is not the Cause of the Rotation: Another may be assigned, simple and easy; but as I have already said too much, it will be better to make it the Subject of another Paper. I am,

S I R,

Your most Obedient

Humble Servant,

Granville Wheler.

II.

S I R,

IN the last Paper I had the Honour of communicating to you, I endeavoured to make it appear, that the rotatory Motion of glass Tubes about their *Axes*, before a Fire, was not owing to the Draught of the Chimney. In this, I beg Leave first to mention an Experiment or two, to shew that the Motion is not owing to any Attraction or Repulsion in the Tubes; and then give the Solution I proposed in my last, but deferred laying before you, because my Paper was already carried to too great a Length.

I suspended two Fragments of small Tubes, 8 Inches long, and about $\frac{1}{10}$ of an Inch in Diameter, near the Fire, from two Pins, by blue Silk Lines, which had each a Loop at one End, were tied at the other to the Top of the Tubes, and hindered from slipping off by a little Sealing-wax. The Tubes came together at the upper End, and receded manifestly from each other at the lower, appearing to be in a State of Attraction above, and a State of Repulsion below: But, suspecting this to be owing to the Sealing-wax, which soon began to melt, I scraped it off both, leaving only as little as was possible, to hinder the Silks from slipping. The Consequence then was, they came together at the lower Ends, and very near so at the upper; and, when suspended from one Pin, so that the Loops of the Silks touch'd each other, the Tubes seem'd equally close all the Way down, without any Appearance either of Attraction or Repulsion. But, imagining still that a repulsive
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Power in the heated supporting Tubes, when placed near together, might possibly be the Occasion of the receding of the upper Tube at Contact with them. To put the Matter out of all Doubt, I wet the three Tubes all over; yet the regressive and rotatory Motion was still manifest, with very little, if any Difference; not more than might be well accounted for, from the Increase of Resistance by Wetting.

These two Experiments fully convinced me, that neither Attraction nor Repulsion would be of any Assistance in solving our Rotation. Upon considering therefore the Matter farther, I found nothing was wanting, but that the moving Tube should swell towards the Fire; and indeed I thought I could perceive such a Swelling in Mr. Orme's long Tube of four Feet and an half, which I saw first placed near a good Fire in the Manner described in my last. For, allowing such a Swelling, Gravity must pull the Tube down, when supported near its Extremities horizontally; and a fresh Part being exposed to the Fire, and swelling out again, must fall down again, and so on successively; which is, in other Words, a rotatory Motion towards the Fire.

When the supporting Tubes are brought near to each other, as well as near to the Centre of the supported Tube, then the Parts hanging over on each Side, being larger than the Part which lies between the Supporters, will, by their Weight, pull downwards, and consequently force the middle Part, resting upon its two *Fulcra*, upwards; and being less advanced towards the Fire, as being less heated, will, by their oblique Situation, pull the middle Part backward also from the Fire: Which Effects, being successive,

cessive, will exhibit a rotatory regressive Motion, quite contrary to what the Tube had when supported near its Extremities: And when a single Tube lies inclining opposite to the Fire, either to the Right-hand or the Left, out of a Plane perpendicular to the Surface of the Fire, Gravity will not permit the curved Part to rest, but pull it down till it co incidēs with a Plane perpendicular to the Horizon; and, consequently, as new Curves are generated, new Motions will be so too; that is, the Tube will be made to move about its *Axis*; but with this Difference, when the Tube inclines to the Right-hand, the Motion about the *Axis* will be from East to West; when to the Left-hand, from West to East. The Justness of this Reasoning is made manifest with a very little Trouble; only bending a Wire, and supporting it first near its Extremities, then near its Centre on each Side, afterwards inclining it to the Right, and then to the Left; the Bending in every Case representing the curved Part of the Tube next the Fire. And that this Solution is the true one, seems farther probable from hence, that when four Supporters were made use of, one at each Extremity, and two near the Middle, there was no Motion at all either backward and forward: Nor is it of any Service to object here, that the Increase of Contact hinders the Motion; because, upon the Plane of Glass, mentioned in my former Paper, so large as to have a much greater Contact with the Tube, both a rotatory and regressive Motion was manifest. I am, worthy Sir, with a high Regard,

Your most obedient humble Servant,

London, April 4. 1745.

Granv. Wheler.

II.